

THURSDAY, JULY 31, 1879

## THE NEW NATURAL HISTORY MUSEUM

**N**OTWITHSTANDING the delay caused by discussions on the Zulu campaign and the Army Discipline Bill, the Civil Service Estimates must shortly come before the House of Commons, and an opportunity will be given for obtaining from the Government some explanation of the course they propose to adopt with regard to the administration of the New Museum of Natural History. As will be seen by the memorial, of which we gave a copy some weeks ago, the Council of the British Association for the Advancement of Science, in accordance with the resolution adopted by the Association at the Dublin meeting, have strongly urged the pressing importance of this question upon the Government. Naturalists, we believe, are one and all of the same mind on this subject, but owing to the many important political questions of the day, and to the general ignorance of, or we may perhaps say, indifference to, the true wants of science, find it very difficult to get their wishes attended to. Their general opinion upon the British Museum question may, we believe, be shortly stated as follows.

The dominant idea, as we all know, of the Founders of the British Museum was the library. The collections of natural history and antiquities which have been added to it during the past fifty years have always been regarded as entirely subordinate, and not-to-be-too-much-encouraged parts of the general scheme. The executive officer of the whole institution has always been the "principal librarian," trained up in the book department, and having his great aim and object to make that department as perfect as possible. Some years ago, in obedience to pressure from without, an eminent naturalist was made "superintendent" of the four sections of the museum which relate to natural history, namely, zoology, botany, geology, and mineralogy, but care was taken to give him no real power, and his authority, we believe, has remained completely nominal up to the present day. The "superintendent of the natural history departments" has never been allowed to interfere in any way with the important functions of the principal librarian, in whom the administrative power of the whole of the Museum is vested. Now fifty years ago, in the infancy of natural history in this country, such an arrangement as this might have answered very well, but with the gigantic strides that science has made of late years, it is not likely that naturalists will be content to allow the great National Museum of the country to continue to be governed by an individual of no scientific attainments whatever, and to be entirely subordinated to the predominant interests of the Public Library. When the Royal Commission on Science was appointed in 1872, and the question of the British Museum came before it, the grievances of the naturalists found vent, and the systematic injustice with which this department of the Museum had always been treated was fully exposed. After taking full evidence on this subject, the Royal Commission came to the conclusion that the objections raised to the present system of government of the natural history collections were "well founded," and were "unable to discover that the system is attended by any compensating advantages." The Royal Commission

recommended consequently that the opportunity should be taken of the proposed removal of these collections into the new building at South Kensington, to separate them at the same time entirely from the control of the trustees and to place them under the rule of a director, who should be responsible to one of the Ministers of State.

It might have been well supposed that such a recommendation, coming as it did from a Royal Commission composed of some of the leading scientific authorities of the country, and backed by the universal opinion of naturalists, would not have been ignored. But such is the apathy displayed by our Government, when questions merely of scientific interest are at stake, that the recommendation appears to have been entirely overlooked. At the fag end of last session the trustees of the British Museum were permitted to pass an act enabling them to move the natural history collections to South Kensington without making any changes whatever in the mode of their administration, and not a single member of the Legislature appears to have raised his voice against this summary mode of dealing with the question.

Unless something can be done to upset the conclusion thus arrived at, it is obvious that the evils so loudly complained of during the stay of the natural history collections in Bloomsbury will accompany them in their migration to South Kensington. The library at Bloomsbury will continue to be regarded as the main business of the fifty trustees, and the natural history at South Kensington will, as of old, be starved in order to feed the wants of the more favoured institution. Besides this, many absurd laws and antiquated regulations exist in the British Museum which it would be highly inexpedient to introduce into a new institution, and which can only be got rid of by a complete change of the ruling powers.

It is said that the trustees of the British Museum, having had the memorial of the British Association pressed upon their attention by the Treasury, are prepared to make certain concessions as regards the management of the New Museum at South Kensington. But in the face of the strong recommendations of the Royal Commission we do not believe that any arrangement of this character will be deemed satisfactory.

In fact, the only hope of good government for the new Museum of Natural History lies in its entire separation from the unnatural foster-sister with which it has been hitherto reared. Every man of science will, we think, agree with the Duke of Devonshire's Commission in considering that natural history has now full claims to a separate maintenance, and will render thanks to the Council of the British Association for their efforts to impress the importance of the recommendations of that Commission upon Her Majesty's Government.

## BRAIN AND MIND

*The Relations of Brain and Mind.* By Henry Calderwood, LL.D., Professor of Moral Philosophy in the University of Edinburgh. (London: Macmillan and Co., 1879.)

THE object of this work, Prof. Calderwood says in his preface, "is to ascertain what theory of mental life is warranted on strictly scientific evidence."

"The order followed is to consider, first, the latest

results of anatomical and physiological research as to the structure and functions of the brain; *second*, the facts in human life unaccounted for by anatomical and physiological science, and requiring to be assigned to a higher nature."

In these words our author indicates not merely his method, but the conclusions as to the relations of mind and brain to which his investigations have led him.

The first six chapters deal with the anatomy and physiology of the brain, both human and comparative. In these and also in other parts of the work Prof. Calderwood exhibits an extensive acquaintance with the facts of cerebral anatomy, physiology, and pathology, worthy of any technical neurologist, and which reflects especial credit on an author hitherto identified with purely speculative philosophy.

As the result of his study of the comparative anatomy and physiology of the brain, he reaches the position that the brains most elaborate in convolution are indicative mainly of the most highly developed muscular system. The development of the brain is, however, no test of "intelligence." This, he contends, is most strikingly brought out by a comparison of the brain of man and the ape. "The ape, with a brain modelled like man's, and weighing 15 to 20 oz., shows himself active, powerful, and able to assail any adversary; man, with a brain better developed, and 10 to 15 oz. heavier, is tottering, feeble, and idiotic, unable to defend himself from even a weak assailant. If configuration and structure of brain afford a measure of intelligence, our poor idiotic fellow-man should be so much clearer in intellect and decided in action than the highest specimens of apes. But it is not so" (p. 161).

The comparison here instituted is a very fallacious one. The exact formula for the relationship between brain development and intelligence in different animals has yet to be found. That it is not a mere matter of size is generally admitted. But that the relationship is thorough-going is proved by the very fact here alluded to by Prof. Calderwood, that below a certain standard of development idiocy is the invariable result. The comparison should not be between a microcephalic idiot and a normal ape, but between a normal ape and a microcephalic one. The microcephalic ape would certainly be idiotic.

At the close of his review of the facts of cerebral anatomy and physiology Prof. Calderwood says: "At this stage it seems our only possible conclusion that anatomical and physiological investigation as to brain and nerve, so far as they have yet been carried, afford no explanation of our most ordinary intellectual exercises" (p. 216).

He quotes with approval Prof. Tyndall's words that "the passage from the physics of the brain to the corresponding facts of consciousness is unthinkable," &c. (p. 212); but not content to accept the two as correlated facts insusceptible of further simplification, he endeavours to prove by "personal experience" that mind is altogether distinct from brain, and of a higher and immaterial nature.

"That we discriminate between sensations and perceptions, and consequently form conceptions of things, are facts towards the explanation of which all that is known concerning the action of nerve-fibres and cellular substance contributes nothing" (p. 224). "The known laws of brain action do not provide for this; they imply that the nerve system is not equal to such work" (p. 221).

In his chapter on "Experience as connected with

Motor Activity" (Chapter VIII.) we find the following account of the nature of volition:—"What we mean by volition or exercise of will-power is best shown, in the first instance, by marking its contrast with nerve-action. It is not that which moves the muscles, but that which moves the nerve-cells to act upon the muscles. It is not that which moves the limbs, but that which determines that they shall be moved. In its lower and simpler aspect this may be illustrated by reference to sensory activity. A falling stick touches the hand, or a neighbour jostles the elbow. By contact with some external body, an impulse is given to the sensory nerve which is transmitted to the sensory cells. Let us now turn to motor activity. In so far as the originating power acts upon the motor apparatus, its action is, in a sense, analogous to that which produces a tactile impression—it operates as an external power, that is, *external* to the apparatus. Or, to take a form of expression more familiar, there comes from an inner sphere, from the region of personal experience, an impulse which acts upon the motor cell, and throws it into activity. That which acts upon the motor cells is as certainly external to the system as is the object which comes into contact with the sensory system. But in the case of voluntary muscular activity, that which operates acts directly on the cell. And what is not reflex, as not being the product of movement of the sensory nerve, must be accounted for by energy from some other quarter, that is, from a sphere external to the nerve system, though within the nature of the person" (p. 247).

Such being the standpoint assumed by Prof. Calderwood in reference to the simplest forms of mental manifestation, it is unnecessary to follow him in his analysis of the higher mental operations.

He advocates essentially the so-called "clavier" theory, that the mind is something of a higher nature and distinct from brain, which plays on brain as on a musical instrument. If the brain is diseased, mental manifestations will be limited or inharmonious, but the defect is purely in the instrument, and not in the performer.

Prof. Calderwood admits that the brain is the organ of the mind; that "a pure independence of mind is not known in our history" (p. 314); that defective development of the brain and idiocy invariably go together; that diseases of the brain are associated with mental derangement (Chapter XIII., "Brain Disorders") and deficiencies (Chapter X. "Use of Speech"); that mind has a powerful influence on body, and that mental work implies physiological waste (Chapter XI., "Action and Reaction of Body and Mind"); and yet, notwithstanding the thoroughgoing correlation between mind and brain which these and similar facts demonstrate, he professes by the aid of personal experience to prove the existence of something distinct from and independent of the conditions of its manifestation. This process reminds one very much of an attempt to lift oneself by the hair of the head, or raise the chair on which one sits. Prof. Calderwood cannot divest himself of his brain by personal experience, nor can he give us any evidence of personal experience without brain.

He acknowledges that "however carefully we study consciousness, we do not thereby attain to any knowledge of the nerve system," and that "only by the slow and

laborious methods of anatomical and physiological research has the human race become aware of the physical conditions of sensory impressions and motor activity" (p. 212).

So far, therefore, as personal experience is concerned, Prof. Calderwood might have equally well relied on it for asserting that sensation and voluntary motion are independent of sensory and motor nerve structures, as for his assertion that mental operations are distinct from the action of brain.

That we are in ignorance of the physical processes underlying many special psychical manifestations may be admitted without invalidating the general fact of their correlation, otherwise clearly established. But to make ignorance on the one side the basis of very positive statements on the other, is to say the least extremely rash. We may not know how or under what collocations of nerve cells and nerve structures subjectivity becomes apparent; but for Prof. Calderwood to exclude it in his definition of the properties and modes of activity of nerves and nerve-centres, and then to argue that personal experience demonstrates it to be something beyond and above, is to beg the whole question.

He very ingenuously estimates the true value of such an argument in a passage, in which he says, "the insufficiency of brain and nerve to perform such work is really involved in the statement of the laws of brain action and the functions identified as belonging to fibres and cells" (p. 1. 122, ital. ours). It would be more logical to reconsider and amend the definition.

Prof. Calderwood's endeavour to prove by scientific evidence the distinct nature and independence of mind, is to attempt the impossible. The utmost that scientific evidence is able to accomplish is to show that cerebral activity and the facts of consciousness are correlated facts insusceptible of further simplification and incapable of being expressed in terms of the other.

Whether we adopt the hypothesis of a duality or a dual unity, is a question of faith, not of scientific demonstration. Science can only deal with the knowable.

Considering the very decided stand Prof. Calderwood has taken on the dual theory in the light of the latest researches in cerebral physiology and pathology, it was not unreasonable to expect some new contribution towards the elucidation of the vexed question as to how the immaterial mind can act and be reacted on by the material body. As to whether they are attuned on the pre-established harmony principle or otherwise, Prof. Calderwood gives us no information. On the whole, perhaps, he has in this exercised a wise discretion. But whatever theory as to the intimate nature of mind and brain may be adopted, the correlation between the psychical and the physical must be accepted, not merely in a general sense, but as regards each individual manifestation. Any work will be gladly welcomed, and will do great service, which serves to throw further light on the relations between psychical phenomena and their anatomical and physiological substrata. Prof. Calderwood's work does not help us in this respect:—rather the reverse. While, as regards the facts of brain on the one hand, and the facts of mind on the other, it contains much that is worthy of praise, as regards their relations it is eminently unsatisfactory.

D. FERRIER

#### SOUTH-INDIAN PALÆOGRAPHY

*Elements of South-Indian Palæography from the Fourth to the Seventeenth Century, A.D.* By A. C. Burnell. Second Enlarged and Improved Edition. (London: Trübner and Co., 1878.)

A WORK like that before us is one of those which make us feel proud of our Indian civil servants. Dr. Burnell has made a name for himself in a field of research peculiarly his own, and the appearance of a second edition of his important work on South-Indian Palæography is a matter of congratulation for science. Apart from the historical and linguistic value of the numerous inscriptions here copied and explained, the light thrown by their decipherment upon an obscure chapter in the history of writing is so important that I shall make no excuse for confining myself to this side of Dr. Burnell's labours, the more especially as this is the side to which he has himself devoted the larger part of his book.

Two questions are brought before us at its outset—the date of the introduction of writing into India and the origin of the South-Indian alphabets. The two questions, indeed, hang very closely together, and the one cannot be completely decided without the help of the other. The earliest examples of writing yet discovered in India are the edicts of Asoka, the Constantine of Buddhism, about 250 B.C. They are written in two different alphabets, and the irregularities they present have been supposed to show that writing was still a recent art. The alphabet of the northern inscriptions, which may be termed the North Asoka alphabet, has been proved by Mr. Thomas to have been derived from an Aramaic original, and consequently to have been introduced by Semitic traders from the Persian Gulf. Dr. Burnell claims a similar origin for the South Asoka alphabet, as well as for a third alphabet used only in Southern India, and known as the Vatteluttu or Old Tamil. Of this Dr. Burnell holds that it "is apparently not derived from nor the source of the Southern Asoka alphabet, though in some respects very near to it."

These opinions of Dr. Burnell have met with a vigorous opponent in Mr. Thomas, who maintains that both the southern alphabets were of Dravidian origin, the Sanskrit alphabet itself being an adaptation of some pre-existing Dravidian one. But it will be difficult to resist the force of Dr. Burnell's arguments based upon the earliest forms of the South Indian characters and their likeness to corresponding characters in the Aramaic alphabets of the fourth and third centuries B.C. As he justly observes: "perhaps the most important proof of the Semitic origin of the two South Indian alphabets is the imperfect system of marking the vowels which is common to them both. They have, like the Semitic alphabets, initial characters for them, but in the middle of words these letters are marked by mere additions to the preceding consonant."

If we once admit with Dr. Burnell that the South Indian alphabets have the same Phoenician origin as most of the other alphabets of the world, we must go further with him and derive them "from an Aramaic character used in Persia or rather in Babylonia." The progress of palæography has made it impossible to derive